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ABSTRACT

The author discusses some reasons for constraints that society places on science. Restraints on scientific inquiry and research are imposed when a scientific pursuit comes into conflict with widely held social values or when the pursuit is questioned in terms of "good science." These restraints reflect different views of the role of free inquiry in society and oppose a tradition of academic and research freedom and contradict the predominant philosophy of science as an open system which is held by the scientific community. Examination of the circumstances which have lead to current rising demands for restrictions on scientific inquiry reveal four components: (1) the scale and rate with which scientific and technological activity has increased make it subject to accountability; (2) professionalization of science and engineering has reached a mature level and can no longer rely on the theory of academic freedom of the First Amendment for protection; (3) the change from an ideology of progress to that of limits or self-denial of technology, is part of the general awareness of limits including limits to natural resources; and (4) changes in attitudes result in a questioning of the theory that all scientific inquiry results in progress. Some ways in which limitations are currently invoked are through requests for impact statements and through lack of financial support. (KC)

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Limits of Inquiry: Straws in the Wind

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From time to time a new conception, and with it a new terminology, comes into view in the social study of science. We all know examples of this sort: "scientific revolution," "modernization," "big science," "multiples" (that is, "multiple discovery in science"), "citation analysis," science indicator." In each case, these point to a new "strategic research site," itself a conception of this kind. Today I shall talk rather informally about some straws in the wind which indicate that the conception "limits of scientific inquiry" or "restraint on scientific research" may be another such term, and with it another such strategic research site. The limit I speak of may be one that appears when a scientific pursuit comes into conflict with other widely held social values, or, more internally, when a scientific investigation is questioned from within in terms of the canons of "good science." As we shall see, we are dealing therefore with a conception that is at its very heart antithetical to the more primitive notions of scientific progress and freedom. Moreover, it is the new form of a very ancient story on the search for the proper limits of knowledge, from the Garden of Eden on. But the new elements may have a qualitatively transforming effect on the science-society relationship which is the special concern of the young organization sponsoring these meetings. In its own search for special purpose and focus, it might well consider making this subject one of

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its distinguishing activities.

It is not difficult to find evidence that the conception of "limits to inquiry" has come to be discussed more and more during the last few years, both within science and outside. I know of several conferences and sessions of professional societies devoted to the topic (e.g., the annual meetings of the AAAS last year and planned for February 1978). A portion of the meetings organized by Senators Kennedy and Javits at Arlie House in Virginia last year dealt with it. It was also the title of a lively faculty seminar that held ten meetings during the last academic year at MIT, with the participation of faculty members in the Cambridge area and commuters from New York, Cornell and Washington. Both the President and Provost of MIT were regular participants, and, in his original letter of invitation, the Provost sounded the basic theme: "The impulse to undertake this study, of which some precursors existed in recent MIT history, has a number of components. Some of these are practical and timely, such as the current discussion concerning the conditions for research on recombinant DNA or on human subjects. Others have their roots in basic, long-term, sociological or epistemological changes. Thus it is now maintained by many that scientists and scholars have long had a bargain with society by which they have produced ideas and devices with few constraints, but that now this bargain is in danger of breaking down or in need of revision. Understanding the extent and reasons for such changes should be of interest in its own right, and may also improve our ability to deal with practical problems some of us are now facing."

Further evidences, of a local sort, of the liveliness of the issue are that the faculty seminar itself was one of the activities of a new entity being

formed within MIT, called the College for Science, Technology, and Society--one of the latest expressions of a movement among universities and colleges--and that the Editor of the quarterly journal Daedalus has decided to devote the Spring 1978 issue to this topic. Papers to be sent shortly to the printer, mostly from members who participated in the MIT faculty seminar, are coming from David Baltimore, Sissela Bok, Harvey Brooks, Barbara Culliton, Loren Graham, Peter Hutt, Leo Marx, Walter Metzger, Robert Morison, Dorothy Nelkin, Don Price, Robert Sinsheimer, Judity Swazey, Lynn White, Jr., among others.

Moreover, a number of professional socities have institutionalized such concerns. One might mention the Forum on Physics and Society of the American Physical Society, and the Committee on Scientific Freedom and Responsibility of the AAAS, whose Subcommittee on Infringement of Scientific Freedom is responsible for the important report prepared by John Edsall in 1975, entitled "Scientific Freedom and Responsibility."

One can of course ask whether the vigor evident in their own researches by the members of this Society, as seen by the great variety of papers scheduled at this very meeting, really calls for the creation of a new research site. It is not out of the question that what we have here is a relatively short-lived excitement, fueled by special events such as the recombinant DNA debate--just as it is possible in principle that we have here something equivalent to the appearance of a Nova in astronomy which causes a good portion of that profession to rush to their telescopes. My feeling is, however, that the situation is somewhere between these limits, that we shall be living with the concep. for some time, and that the subject is precisely one where the combined

interests of scholars in the history, sociology, and philosophy of science can find a common focus. It is therefore worthwhile to sketch today a rough map, indicating perhaps only in a few sentences where the possible research areas appear to lie, without claiming in the least that we are already in a position to talk about "solutions to the problem." At a later time, someone might then wish to attempt an analysis, perhaps along the lines of Robert Merton's paper in 1963, in which he gave eight good reasons why it would be useful to make a methodical study of another research site, that of multiples.

I.

The first of the four points I wish to make is this: "Limits to inquiry" may be a useful set of spectacles through which to re-examine old cases. Once the term has been put before us, one cannot help but notice a curious asymmetry. Social studies in science, in the vast majority of cases, have been concerned with scientific advance, progress, triumphs, including triumphs over the human and institutional limits; with increases in the rates, proliferation of directions, enlargement of content and power of scientific work. Limits on these have been discussed in most cases in terms of interventions from inside and outside science, in terms of impedimenta in the development of intellectual progress or of science policy.

The constraints society places on science is a rather old story; it is more rare to find works on the resistance to new ideas and to new technologies examined on its own grounds, with attention to its functionality and dysfunctionality. Other limits, restraints, delays, diversions of scientific work, and the corresponding, mirror-like opposites which show up in the social

processes during that interaction with science--those are even more infrequently attended to, although, happily, we have several of the exceptions right in this room.

The accepted ethos and practice of science does not, in fact, deny limits or constraints, as you well know. On the contrary, a number of such constraints have been found essential: that peer review be undertaken on funding priorities and on the soundness of procedure; that informed consent be obtained; that quantitative rather than qualitative results are preferred where possible; that operational definitions rather than metaphysical ones be used; that experiments be repeated; that an attempt at relating theory and practice be made; that the work be ultimately published; that credit for priority, collaboration, and support be given, and so forth. As every modern researcher knows, getting time, funding and manpower to do an experiment involving one of the large machines, or the approval of the local committee on regulation of hazardous biological agents, can lead to epic battles.

But these self-imposed limits among scientists, and even those self-denying ordinances from Hippocrates to Leo Szilard, are of a different kind from those now being discussed. Equally important, the constraints listed above are largely tacitly accepted rather than being the subject of conscious examination or even of explicit pedagogy. The very least one may therefore expect from attention to the topic of limits of inquiry in its modern phase is that there will be a re-invigoration of studies made of scientific practice, somewhat like re-using existing data at relatively little cost.

II.

My second point: We are not talking about an academic exercise, and not only about historical cases (although these might be among the

most interesting), but about a battle going on in real time, perhaps on your TV screen tonight. In fact, there are several battles, involving fundamentally different views of the role of free inquiry in society.

To illustrate: As you know, a laymen citizens' group was appointed by the City Manager of Cambridge to help his City Council decide on guidelines for the use of recombinant DNA-molecule technology. When the citizens' group submitted its report last January, it declared itself as follows, on page 1: "Throughout our inquiry, we recognized that the controversy over recombinant DNA research involves profound philosophical issues that extend beyond the scope of our charge. The social and ethical implications of genetic research must receive the broadest possible dialogue in our society. That dialogue should address the issue whether all knowledge is worth pursuing. It should examine whether any particular route to knowledge threatens to transgress upon our precious human liberties. It should raise the issue of technology assessment in relation to long-range hazards to our natural and social ecology. Finally, a national dialogue is needed to determine how such policy decisions are resolved in the framework of participatory democracy."

In the body of the report, one encounters similar concerns (for example, that limits should be placed on "elite" decision-making concerning scientific programs, and that limits must be placed on research in which there is fundamental ignorance about risks). Vannevar Bush's euphoric and ruling conception of Science as the Endless Frontier seems here to be giving way to the notion of Science as the Suspected Frontier. This is an example of one battle, that between the concerned citizen on the one hand, and the large majority of scientists

who would not agree with the implications in the passage quoted.

The second battle is that within what is lightly called the scientific community. (If the first was characterized by the interchange between Theodore Roszak and Steven Weinberg in the Summer 1974 issue of Daedalus, the second will be indicated in the exchange between Robert Sinsheimer and David Baltimore in the March 1978 issue). On the one side are the scientists--a minority, ranging from earnest and eloquent scientists of the old style to a shrill, radical cell--who would at the very least agree with the remark in the AAAS Committee report referred to before, when it says on page 1: "Some of the consequences of science and technology frighten, discourage, and even horrify most of us." Prominent spokesmen on the urgent issue of DNA research have been Erwin Chargaff of Columbia University and Robert Sinsheimer of California Institute of Technology. Sinsheimer put it this way, not long ago: "We begin to see that the truth is not enough, that the truth is necessary but not sufficient....When we are armed with such powers I think there are limits to the extent to which we can continue to rely upon the resiliency of nature or of social institutions to protect us from our follies and our finite wisdom. Our thrust of inquiry should not too far exceed our perception of their consequences....We need to recognize that the great force which we now wield might--just might--drive us too swiftly toward some unseen chasm."

Sinsheimer went on to question "whether in particular instances, we want to know the answer in every case. Given the nature of man and of society, are the secondary consequences of such knowledge, on balance, likely to be beneficial? Here, it may be the highest wisdom to recognize

that we are not wise enough to know what we do not want to know, and thus to leave the ends of inquiry unrestrained." He goes on to give four selected illustrations of research in that category, where he believes at this time our society may receive appreciably less advantage than possible harm from the pursuit of progress of investigation: contacts with presumed extraterrestrial intelligences (which, if successful, might have a shattering impact upon our civilization); research on cheaper methods of isotope separation (which are very likely to proliferate nuclear weapons); research on predetermination of the sex of children (with its supposed potential for unbalancing the natural sex ratio); and, lastly, indiscriminate research upon the aging process (which threatens to add decades at the decrepit end of life). These are by no means idiosyncratic examples; others such as Martin Ryle and Hans Jonas have, to varying degrees, also spoken up for placing limits on inquiry in specific cases, quite apart from the DNA controversy.

One could well add here analogous remarks on the third and fourth battle: for it is really a four-cornered situation, in which the participants are a) scientific professionals tending to argue against limits; b) scientific professionals arguing for limits; c) nonscientists arguing against limits; and, d) nonscientists arguing for limits. The controversies can be discussed in terms of those between a) and b), c) and d), b) and c), and a) and d). The largest group of course belongs to the first of these four categories. That is, the center of gravity is in that part of the scientific profession which has and can have no other response than to preserve the integrity of its activity against what it perceives to be a potentially very serious

threat from an unchecked limits-to-inquiry movement. Whether from citizens or fellow scientists, any calls for limits go against explicit long-standing traditions of academic and research freedoms, absolutely defined, as still perceived by most scientists. They contradict the predominant philosophic base of science as an infinitely open system in which it is not to be feared that we shall run up against in-principle unanswerable questions, not to speak of unaskable ones. It is contrary to the psychobiographic drive of most of the younger scientists and the reward system that has shaped the older ones. It clashes violently with the world view of scientists brought up on the notion that science and optimism are virtually synonymous, that somehow the doing and the finding of science will automatically be for the good of mankind--for science's own sake, for its anti-metaphysical and liberating philosophic message, and for its eventual technological fruits (a view now popularly held in the second- and third-worlds more firmly than in ours).

One recalls Jacques Monod's remark in Chance and Necessity, that the ethic of knowledge is the commitment to the scientific exploration of nature, and P. W. Bridgman's famous remark that the scientific method is "doing one's damndest, no holds barred." The limits movement conflicts basically with such commitments, as it does with demonstrable national and international needs for a new knowledge, even sometimes of a "risky" kind, to ameliorate patent human misery. Above all, it comes ironically at a time when the results of scientific and technological work, both in terms of quantity and in terms of quality, contain more examples of ingenuity and beauty, by any standards, than ever before.

As a consequence, scientists who are in favor of as unrestrained an autonomy as is compatible with safety in circumstances of demonstrable risks tend to say that the very pursuit of such questions as limits may be self-fulfilling, and indeed over-fulfilling. For example, the president of the National Academy of Sciences was recently quoted as having said, "Particularly troublesome is the ever-more frequent expression of the notion that there are questions that should not be asked, that there are fields of research that should be eschewed because mankind cannot live with the answers. Nonsense. No such decision can be rational, much less acceptable." James Watson was recently quoted: "All I can say is that we want to go on with what we are doing, and I don't think we are crazy." Nonscientists who have warned scientists against accepting externally imposed limits to inquiry include Gerard Piel and George Ball. Many more will be heard from.

To help sort out the arguments, Loren Graham has produced a useful taxonomy, somewhat in the style of Linnaeus or Mendeleeyev, a taxonomy of concerns about science and of attempts to regulate inquiry. In his draft paper, to be published in March, he concentrates in good part on the comments obtained in a 1974 NSF-sponsored survey, in which 39% of the sample of the U.S. population expressing any opinion did not agree with the proposition that "Overall, science and technology do more good than harm," and in which 34% of the population sample expressing any opinion felt that "the degree of control that society has over science and technology should be increased." He discerns nine broad categories of concerns, some of which have sub-categories. One set is "concerns about technology": 1. Concerns about the physical results of technology (in

shorthand language, "destructive technology"). 2. Concerns about the ethical results of technology, especially biomedical ethics ("slippery-slope technology"). 3. Concerns about the economic results of technology ("economically exploitative technology").

The second set are concerns about science: 1. Concerns about research and human subjects ("human subject research"). 2. Concerns about distortions in allocation of resources for science ("expensive science"). 3. Concerns about certain kinds of fundamental knowledge, e.g., knowledge itself ("subversive knowledge") and knowledge "inevitably" leading to technology ("inevitable technology"). 4. Concerns about accidents in the research itself ("accidents in science"). 5. Concerns about the use of science to excite racial, sexual or class prejudices ("prejudicial science"). 6. Concerns about certain modes of knowing (e.g., those "alienating" for the human spirit).

These categories are fine-structured enough to encompass the responses not only from the man in the street but from some scientists on the one hand, and highly placed nonscientists on the other. Thus, the last of the categories would perhaps characterize the recent draft statement from the World Council of Churches which opened with the following words: "However robust the faith of yesterday in the power of science and technology, it is today misplaced and therefore misleading. In fact, the reductionist, triumphalist, manipulative approach of science and technology has itself been in large part responsible for our predicament."

III.

Third point: The circumstances under which limits-of-inquiry questions appear are very widely varied, so widely that regardless of one's primary expertise in history or sociology of science, it should not be difficult to

find a congenial real case to work on.

Let me mention three examples. The most obvious is of course the set of cases involving limits placed on experiments involving the human being as the animal of necessity. The conditions under which research may involve prisoners, children, or pregnant women is the subject of a stream of regulations whose purpose and legislative history are important research sites. Moreover, they lead to illuminating cases when they result in conflicts, e.g., when there is a breech of the rules, knowingly or even unknowingly (as in the recent case involving the team isolating a rat gene which codes for insulin, causing the experiment in progress to be hastily aborted when an unintended breaking of the NIH guidelines was discovered). A case that has some similarities is that of the National Security Agency asking for the cancellation of a symposium on Cryptology, scheduled a few days ago by research mathematicians, on the grounds that the open discussion of research in that field may constitute a technical violation of the State Department's international traffic-in-arms-regulations and possibly other secrecy laws. ¶ Another very visible area involves calls for limits when the risks feared are not simply physical or biomedical. As Salvador Luria has written recently: "The issues raised by biological research in the field of social science are more subtle. Damage can be done indirectly through the functioning of social forces and institutions." He goes on to give his own stand: "If a single person, black or white or yellow, should suffer because of the social consequences of the right claimed by some psychologist to find out what a 15 point mean difference in IQ really means, then maybe we should forsake that knowledge. That truth may not be worth such a price.

If the life of a single XYY child or its parents should be blighted or anguished because of the wish of some psychiatrists to know the true facts about the psychological XYY pattern, again, I would caution that truth may not be worth such a price."

He names this stand the ethic of innocence, counterposed to the ethic of knowledge. A similar approach characterizes a certain section of those who would limit research on human sociobiology. It is noteworthy that from within scientific societies, petitions to ban or not to fund research in sociobiology have been presented at the recent meetings of two professional societies, that for anthropology last year, and that for sociology this last September. I would expect that a social-studies-of-science approach to this case would yield raw materials as significant for research as Charles Weiner's group has been obtaining on the DNA controversy.

My own interest goes primarily to the discussion surrounding the possible introduction of impact statements. Scientists and their institutions have by now routinely accepted their liability for impact statements dealing with environmental impact, financial responsibility, manpower policy, affirmative action, and the like, in connection with the funding and research procedures involving science and engineering. There is now a certain current of opinion that an ethical-and-values-impact statement might well be added to these in certain areas of inquiry. To a degree this was the message of the Bauman amendment in the last Congress by which the Congress would reserve for itself the right to re-examine each application for which an NSF grant has been made after peer review, to determine the effect or

impact this research might have, in the opinion of the Congressmen concerned, with a view to putting an embargo on particular grants. In a similarly motivated action, the authorization bill for appropriations for the NSF issued last March directed the NSF to provide for the protection of students in tryout classes that use pre-college educational materials. The aim was to make sure that "interested school officials" could make certain that steps had been taken "to insure that students will not be placed 'at risk' with respect to their psychological, mental, and emotional well-being" while serving as human subjects in these educational developments.

The idea of asking applicants for a statement of the prospective impacts of the research has been quietly circulating in some foundations and and universities. Thus, in March 1974, a faculty committee was set up at MIT to see how faculty members could make a more thorough and self-conscious study of "the impact of MIT research insofar as that research has influence on matters such as the physical environment, the economy, national security, and other important social concerns." One function of the committee was to develop methods "to assist individual investigators in the preparation of impact statements." In making its report to the faculty, the committee said "An honest effort to estimate the plausible consequences of his own research is as properly to be expected from a researcher as now he is expected to estimate the cost of carrying out his work, and is in the long run perhaps more significant."

It was the report of a sober committee, under the chairmanship of John Deutch and with members of the calibre of Frank Press--both now having

prominent positions of scientific leadership in Washington. The faculty thought at the time it was not ready for the experiment. When it came to a vote, the plan was defeated, but with a statement by MIT President Wiesner that the issue should not be dropped, and that a more specific proposal should be developed for future action. I expect we shall hear more about this and, more important for our purpose, that this area will turn out to be a very fruitful research site.

A third set of examples concerns the most ancient way by which inquiry has been limited: lack of support. Of these, the simplest has been the barring of researchers, largely on political or philosophic grounds. Before this audience, there is no need to recite the cases from Galileo and the followers of Darwin to the scientists in the Soviet Union today, who, as in the case of Benjamin Levich, find their very names deleted and left blank in the pages of scientific journals from the U.S. when these are photographed, edited and distributed to scientists in the Soviet Union by their own authorities. There is also the testimony, until lately rather overlooked by historians of science, of the victims of restraint on inquiry in the case of women scientists (as recounted, for example, by Otto Haas in his autobiography, in which he details difficulties Lise Meitner had in her early career); by the victims of the numerus clausus, that affirmative inaction device by which many regimes throttled the flow of imaginative people into science; and of course of the tragedy that befell scientists in our century, from the Nazi period through the McCarthy witchhunts to the hundreds of scientists languishing at this very moment in the prisons of Chile, Argentina, and other countries.

Less brutal, but in its way equally effective as disbarment or harassment of scientists, is the under-financing or abandoment of certain fields of research on political or philosophical grounds. An instance here concerns the field of reproductive biology. The president of the World Bank, Robert S. MacNamara, is not known as a man of passion and fervor; but some of those emotions could be read into his address of last April in which he pointed out that despite the great urgency and the number of promising avenues of fertility regulation, "the field has been so starved for funds in recent years that more than half the approved grant applications for reproductive biological research have simply failed to be financed," and that the world-wide expenditures are several times lower than what is urgently required. On the other hand, an embargo on inquiry that may have much more cheerful results may come from the current discussion among well-placed arms control specialists that the time has come at long last to try to negotiate arms control agreements before some new overkill system has been developed, paid for, and is in place or ready to be deployed. That is, it may be possible to get limits put on the inquiry process that specifically yields weapons developments in the first place, e.g., in research dedicated for the purpose of producing powerful lasers and particle beam weapons. Putting restraints on applied research of this sort is of course in principle a great deal harder to enforce and monitor than putting limits on the building of large ABM installations. Nevertheless, the case is not at all hopeless--the ill-fated ban on atmospheric test explosions was one such attempt--and the gain could of course be spectacular.

V.

Now to my last main point: As our eye passes over these various and separate evidences of constraints to be found in effect here and now or on the horizon, we are naturally led to ask what the theoretical substructure may be of which these are concrete indicators, somewhat as the observable movement of the straws can tell us something about the wind current itself.

Even if all too briefly, I can propose here four components of that current.

1. Scale and rate. Most obviously, the increase in the scale of scientific and technological activity in this country and in some others has made for visibility, and with that--as should be the case in a democracy --has come a re-examination of the mechanisms of accountability. The annual cost of basic research in the United States is now close to $5 billion, two-thirds of it supplied by the Federal government, and a total of nearly $40 billion are being spent on the national research and development effort (including defense and space projects, which the public identifies, for better or worse, closely with science). Clearly, any enterprise that employs of the order of a million scientists and engineers, and commands 15% of the relatively controllable portion of the Federal outlay, must be subject to mechanisms of accountability with respect to its performance and justification, in terms that the taxpayer or his representative can appreciate.

One byproduct of this has been the initiation of science indicators. Another has been the perception that the level of various risks is rising,

compared to the level associated in the past with characteristically slower technological change. And, again, while outright abuses and visible mistakes constitute a small portion of on-going work, when the rate of work is large enough, that small proportion of identifiable abuses does cumulate to form a large enough set of cases in absolute terms, adding again to the visibility.* Finally, as the rate of change of scientific knowledge and of technological advance has increased, it has had the ominous effect of decreasing the time interval during which the change being imposed can be monitored, evaluated, and absorbed intellectually by a public that does insist more and more on having a role in consequential decisions.

¶One result in the increase in scale and rate, then, is that new accountability systems are being fashioned by the scientific establishment and

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*It would be interesting to study the influence on the "limits of inquiry" movement of the perception of abuses and mistakes, surprises and unexpected problems that, correctly or not, have been seen to attend the "progress" of science and technology in our time. One thinks here of a whole bouquet of horrors, from the effects of thalidomyde to the side effects of pharmaceuticals containing estrogen and the lack of vigorous research on alternatives to mastectomy. One also thinks here of the West Ford experiment, or the injection of electrons into the Van Allen belt by means of nuclear explosions, or the dubious mathematics of risk calculations in certain nuclear-reactor assessments. If one studies popular perceptions of science, one finds that in the back of many a mind there is a dim awareness that with 25% of the world's scientists and engineers engaged fairly directly in weapons-related research, and with many others obeying the imperatives of their commercial employers rather than the canons of academic inquiry, the visible problems take on the role of "indicators" of fearful possibilities, And of course one also cannot forget the betrayals of the Hippocratic Oath by the German doctors as told at Nuremberg and, more recently, by those who did their reckless experiments on behavior control under CIA auspices.

by the representatives of the public, in mutual negotiations which Dorothy Nelkin has been studying. These groups are trying to devise new mechanisms of great intellectual interest, centering for example not chiefly on accountability with respect to their peers, as heretofore, but with respect to those who are at risk, or their representatives.

The old model, the old "bargain," was really a quid quo pro and barter of an odd, unengaged sort; that is, society expected to get benefits from the work of researchers in science and technology, and those in turn received financial support and administrative freedom from society. In that barter, each side was giving something it could well afford or at least did not particularly treasure, and there was little to understand beyond the gains each side was receiving for itself. It was a system that worked well, and is moderately well understood. The new model, however--again in barest outline--involves the giving up by each side of substantial, even treasured items, and the exchange requires much more mutual involvement and understanding. Each side now has to barter away some of its autonomy, and each side expects to obtain substantial benefits and assurances. The whole argument about where the limits of scientific inquiry are is a symptom and byproduct of this larger encounter.

The mood I see developing in this encounter corresponds to the impression I gained at the end of the series of MIT seminars. If there was any consensus there, it was, in the words of one of the authors, that just as we are wary of the 'slippery-slope' in biomedical ethics, so we should resist slipping inadvertently into increased controls over fundamental science, since such controls can easily lead to abuses of their own.

The image shared by most scientists there was that they are therefore dealing with two slippery slopes, joined at the top by a razor's edge upon which they are precariously balanced. There will thus be hard bargaining on such questions as these: Who will control what?, and at what level? (At the level of the institution? of the individual researcher? of funding?) How can "public participation" be arranged without clashing with the very meaning of science as a consensual activity among trained specialists? (In the old bargain, the inherent contradiction between science and democracy did not have to be faced; now, it has to.) When the largely unwritten code of ethics maintained by peer pressure is turned in for a set of Congressionally developed guidelines, how can enough room be left for personal choices, as conditions and knowledge change, against a backdrop of blanket regulations formulated by bureaucratic entities with considerable inertia? (This is analogous to obtaining "variances".) And, above all, how can the feeling of freedom and momentum be preserved upon which any imaginative group must be able to draw to do its best?

While the barons of the scientific establishment are engaged in these, sometimes rather public, negotiations, those in the lesser ranks are left to cast about for their own new roles. For some of them, this takes the shape of attempting to define individual social responsibility of the scientists in this new age. Others--perhaps the smallest of the groups, but the one I am most interested in--will recognize that this whole area is in greatest need of research and communication among like-minded researchers. Thus, the assessment of risks or benefits is possible at this time only at very short range, the rationale of setting limits will

often require transgressing them in the first place before they can be properly determined, etc. For still others, and again, of interest to me personally, there is the challenge to build into the education of future scientists and engineers some understanding of these issues. That topic, too, is as urgent as it is complex.

2. New maturity. One may argue that the recent developments which brought the limits-to-inquiry discussion to the fore also indicates that the professionalization of science and engineering is reaching a more mature level. For it has been the case that while most professions develop a system of public legitimization and certification, the scientists, and to some degree the engineers, have been largely exempt from this, partly because of their previously rather low visibility and low degree of coupling with the larger social processes. At this late date, scientists are about to discover that they are, after all, not going to be able to rely in their activities as experimental researchers on the protection either of academic freedom or of the First Amendment. ¶ As to the former, Walter Metzger of Columbia University has analyzed in detail the very limited degree to which traditional academic freedom protects such activities, and how historically wrong it is to confuse what scientists now want to do with traditional Lehrfreiheit--something that referred to the freedom German professors had for setting their own course syllabi even though they were really in the civil service. Nor, of course, can one conjure up Akademische Freiheit, since it referred not to the freedom of the professors at all, but to that of the students, i.e., to Lernfreiheit. As to the other claimed protection, Harold Greene, the head of the AAAS

Subcommittee of the Committee on Scientific Freedom and Responsibility, in a recent article in the Harvard Newsletter on Science, Technology and Values, and also in his testimony before Representative Thornton's House Committee of the Committee on Science and Technology, confessed that it was very dubious that laboratory experiments are under the guarantee of the First Amendment, and they surely are not if they are believed to be injurious to public health and safety.

3. The third main force driving events which strike our eye is the change now underway in the developed countries of the West from the old Ideology of Progress to a new Ideology of Limits that goes much beyond limits to a scientific inquiry. Thus, one of the lessons of the decision not to build the SST in the U.S., not to build the B1 bomber, not to go to a plutonium economy, is that it may now be possible to invoke an almost unprecedented self-denial of technology, a turning off of the "can-do = must-do" imperative. I would predict that future sessions of this Society will have lively papers on the non-emergence of possible technologies and applications. To a certain degree, the embargoes on science proposed by Sinsheimer can be characterized as being calls to stop pursuing possible knowledge of how-to (rather than knowledge of why), and are therefore calls for that self-denial of new technology.

This development fits in with the general, new awareness of existence of limits (the word that may yet come to characterize a main preoccupation of the '70s): limits to natural resources and, in particular, energy supplies; limits to the elasticity of the environment to respond beneficently to the encroachment of the man-made world; limits to food supply; limits to

population; and, within science itself, limits of the kind which Derek Price was among the first to point out in the early '60s with respect to the growth of the science establishment. The pervasiveness and reality of this new climate of opinion is being more and more widely studied. In a major report made for the Joint Economic Committee of the Congress, entitled "US Economic Growth from 1976 to '86: Prospects, Problems, and Patterns," volume 7 is devoted to Limits to Growth, and it specifies the existence of a number of new, popular "emphases and trends" that are expected to influence growth during the next decade. These include the continued rise of risk aversion, concern with health and environmental protection, and anti-technological and anti-industrial attitudes. In all this, the image has been changing for a substantial part of the population of the West, from the century of frontiers waiting to be exploited, to the century of the globe as a crowded lifeboat.

4. Finally, there is a fourth and perhaps decisive component in the current shaping of events, and that has to do with the rise of a new conception of Progress in science itself. Here, too, a substantial change in Zeitgeist has been taking place, and it surely reinforces the effectiveness of the limits-to-inquiry movement mobilized from outside science and technology.

To back off for a moment: To be sure, the old notions of the purity and objectivity of science and of its claims to truth in some absolute sense have been under attack for a long time (as most recently studied by J. Ravetz), and Daniel Bell and Joseph Ben-David among others have written about the resulting loss of the ancient charisma associated with science. But this

had little effect on most research scientists, whom such epistemological and perhaps even to some degree quasi-theological developments reach with a long-time delay. They are engaged in an enterprise in which they consider that progress and cumulation are perfectly well identifiable, that they are putting together the one story which is not a Rashomon story. The chief earmark of progress is still taken to be the classical one: greater inclusiveness of separate subject matter, and greater parsimony or restrictiveness of separate fundamental terms.¶ Newton's Principia would still serve as an exemplar of that kind, for example, in its theory of lunar motions. For Newton's gravirational analysis not only allowed the prediction of the periods and magnitudes of known "inequalities," but showed the necessity of previously unsuspected inequalities, derived from the theory but not available from previous observations. The vision created by Newton's work was that the concepts of force and mass, and the efficacy of mathematical and observational techniques, sufficed to discover an overwhelming unity across an immense spectrum of observables. Filling in that promise of unity largely defined progress.

In our century, the incorporation of Newtonian mechanics into relativity, and of special relativity into the general theory of relativity, were further examples of progress through these techniques. Again, at the end of his Principles of Quantum Mechanics, P. A. M. Dirac confidently declared that since "quantum mechanics may be defined as the application of equations of motion to atomic particles," the domain of applicability of the theory "includes most of low-energy physics and chemistry." The implication was

that these are essentially solved problems, the past rather than the future of science. The latter still beckoned: Dirac wrote, "Now there are other kinds of interactions, which are revealed in high-energy physics and are important for the description of atomic nuclei. These interactions are not at present sufficiently well understood to be incorporated into a system of equations of motions....[But] it is to be hoped that with increasing knowledge a way will be found for adapting the high-energy theories into a scheme based on equations of motion, and so unifying them with those of low-energy physics."

Another example of this model of progress by inclusiveness and parsimony--which few if any scientists can or should resist--can be found in almost any major article in the latest issue of one's professional research journals. A more accessible glimpse of the same point is obtainable in, for example, an article of Victor Weisskopf, called "Of Atoms, Mountains, and Stars: A Study in Qualitative Physics" (1975). Weisskopf starts by taking six magnitudes of physical constants as fundamental and known from measurement: the mass of the proton, the mass and electric charge of the electron, the light velocity, Newton's gravitational constant, and the quantum of action of Planck. He uses these, together with a few fundamental laws (e.g., deBroglie's relations concerning connecting particle momentum and particle energy with their wavelength and frequency, and the Pauli principle), and derives a host of apparently unconnected facts, known to us separately, by observation: for example, the size and energy of nuclei, the mass and hardness of solids such as rocks, the height of mountains on earth, and the size of our sun and similar stars.

This is indeed Newton's program, triumphant. If any one of the six elements on Weisskopf's list, now considered to be separate, is found to be subsumable under the other five--for example, if the electric charge of the electron were found to be deducible from some of the other fundamental constants--then one would know that one is "progressing" (and, in such a case, an immense distance).

The development of the notion of progress may be characterized as de-mythification (in the sense that since about a century ago all but traces of a coupling between scientific progress and moral philosophy or theology have been lost). But other than that, the typical research scientist's fundamental attitude to progress, at least in a work of synthesis, did not substantially change. He typically also remains generally under the impression that the criteria of successful work are largely internal to science, and that the effects upon society, both technological and in terms of changes and world view, are relatively slow, therefore digestible, and in any case unforeseeable but also beneficent.

Now, it is significant that until lately the conception of science as inexorably "progressing" has been a component of thought also in the historiography of science. George Sarton went so far as to assert "The history of science is the only history which can illustrate the progress of mankind. In fact, progress has no definite and unquestionable meaning in other fields than the field of science."

More recently, Thomas Kuhn's widely read view of science has made a claim that looks similar: "Viewed from within any single community, however, whether of scientists or of nonscientists, the result of successful creative work is progress. How could it possibly be anything else?" But in this case there follows a disturbing implication that has been the subject of much

discussion: progress is illusion. For while "progress seems both obvious and assured...," the scientific community "could view the fruits of its work in no other way." And, again, "solving those problems must inevitably be progress," not only in ordinary science, but even in "revolutions"; for to the side that carries the day, the "outcome of revolution must be progress." The perception of progress "is, in important respects, self-fulfilling." The upshot is that we may "have to relinquish the notion" that the large changes which science occasionally undergoes "carries scientists and those who learn from them closer and closer to the truth."

Now it is clear that if it is widely believed that scientific progress is a tautology, then limiting some specific work, in the presumed interest of other more urgent human values, will seem far less of an intolerable intervention. This is not to imply that the Mayor of Cambridge, in wanting to stop DNA research here, was motivated by reading Thomas Kuhn's book. Rather, the theory of progress in that book seems to me to be part of a wider current, in which working research scientists are now 'deviants' in their actions, even if not always in their words.

Nor can we find a very different solution proposed by the most vocal opponents of Kuhn's views, the ambitious school that attempts to analyze the conception of progress, centered on the philosopher of science, Imre Lakatos. He has repeatedly and frankly announced his goal. In a recent (posthumously published) discussion in Minerva, Spring 1976, Lakatos identifies his own school of thought, and especially his "methodology of research programmes" as a version of "Demarkationism." A chief role of the "demarkationist philosophy of science" is to "reconstruct universal

criteria" which help to distinguish "progressive" research programs from "degenerating" ones. Thus, Lakatos warns, not only "medieval 'science'" but also "contemporary elementary particle physics and environmentalist theories of intelligence might turn out not to meet these criteria. In such cases, philosophy of science attempts to overrule the apologetic efforts of degenerating programs."

Lakatos gave the label "Elitism" to the contrary conception, i.e., the notion that in distinguishing betwen worthy and unworthy work ("progress and degeneration, science and pseudo-science"), the "only judges are the scientists themselves." That idea he scorned as stemming from an "undemocratic, authoritarian" school of philosophy, whereas his own Demarkationists "share a democratic respect for the laymen." That layman, and particularly the lawmaker as layman, has to be helped, of course. For this very purpose, Lakatos says, "A statute book, written by the demarkationist philosopher of science, is there to guide the outsider's judgment." But guidance leaving perhaps too much to chance, direction is safer, after all: the Demarkationists, Lakatos announced finally, will "lay down statute law of rational appraisal which can direct a lay jury in passing judgment." It is a breathtaking ambition. Far from being dismissed out of hand, it was reported recently that one major private research-funding agency in the U.S. for a time considered using the idea in selecting among its applicants' proposals.

Having identified the "correct" way for scientists to proceed and to avoid what he called degenerating programs, Lakatos briefly discussed (in an article in the recent book edited by Howson) what to do in order

to eliminate them. Taking the simple approach of the dedicated person who is used to stopping activities with which he disagrees, he gives straightforward directions on how to limit such inquiries. Of the scientists who continue to publish in this disapproved manner, he says, "Editors of scientific journals should refuse to publish their papers.... Research foundations, too, should refuse money." So here, too, one runs directly into a theory of progress that leads, this time explicitly, to prescriptions for limiting inquiry.

I believe we are in a period where old and new theories of progress in science are competing, and,as you would expect from me, I have been trying to understand these competing theories in terms of antithetical thematic trends. This, however, is not the time and place for that discussion. For today, I hope I have demonstrated to at least a few members of this young Society that the wind currents are showing that an interesting storm appears to be developing, and that it might be time to go out with a student or two to keep a watch on it.

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